HISTORY and PHILOSOPHY of SCIENCE in an UNDERGRADUATE PHYSICS COURSE

By Norwood Russell Hanson

FOR five years a new subject has been taught within the Natural Sciences Tripos at Cambridge University. I have been associated with the development of this course, and I should like to tell you something of our objectives, and of the difficulties we have experienced.

Before I even pose the question of whether or not there is anything to be gained from stressing historical and philosophical factors in the teaching of science, I had better say something about the notions of history and philosophy themselves. This preliminary is important. A remarkable amount of indifference, even opposition, to this new approach to science teaching derives from misconceptions as to just what historians and philosophers find interesting in science. My first task then will be somewhat negative in character; I wish to make it clear what we at Cambridge do *not* do.

Historians of science are more than chroniclers. Singer, Sarton, Thorndike, Nicholson, Butterfield, and my colleague A. R. Hall are not concerned just to construct a master record of what happened and when —of discoveries, inventions, and scientific personalities, of birthdays, and family connections. True, many books on the history of some science read as if the author were designing a kind of periodic table, or calendar, or genealogical tree of the events which have made the science what it is. But this is to history of science at its best as bird watching is to genetic theory.

History of science is concerned with ideas—with the thinking of scientists. And this is also what the philosopher of science is interested in, only in a different way.

A consistent opposition to the study of history and philosophy of science ought to begin by denying that scientific research requires any thought at all. For once it is admitted that doing science does require thinking, it is clear that two related studies are immediately important to an appreciation of that thinking. Thinking evolves, and it has an internal structure. The historian explores the evolution of scientific thinking and ideas. The philosopher explores the internal structure of scientific thinking and ideas. This is only generalized from what obtains when any scientist is said to have an adequate grasp of a certain idea. Would we ever say this of a man who lacked *all* knowledge of the development of an idea and all knowledge of its internal structure—its logic?

So the historian of science is not a Royal Society book-keeper, kept to settle future claims as to the priority of inventions and discoveries. He is an explorer. He seeks those factors in the intellectual environment of a given period which led to the formation of a certain pattern of thought. He wishes to disclose new dimensions in concepts such as *acceleration, force, mass, charge, field, point,* etc. He does this by revealing factors which inclined men of different scientific periods to fashion these concepts one way rather than another, this way rather than that. Just as we can understand a man's career better when we know something of him—how he has behaved on similar occasions, and why, what his views are on the matter which led to his action, etc.—so we shall have a better grasp of a scientific concept, e.g., H_2SO_4, when we know something of what led chemists to express themselves in this way with respect to this substance.

It has been remarked * that the formula H_2SO_4 contains the history of mankind. That may be an exaggeration. But it must be clear that insofar as science masters and university lecturers expose this formula to their students as if it were but a token in an intricate juggling act called "chemical theory", they are losing an opportunity to put a broader, more exciting interest into their presentation of the subject. What is more important, they are failing to disclose the full significance of the formula H_2SO_4 within the system of chemical theory itself.

Notwithstanding an indifference on the part of some scientists to history of science as a discipline fit for undergraduate consumption, few would deny that the sciences *have* a history. Philosophy of science, however, does not always escape with even this minimal claim. Since I am a practioner of this black art, I had better proceed to do in detail for philosophy of science what I have done cursorily for history of science. As before, I will begin by saying what philosophy of science is not, or what it need not be.

IF history of science is not chronicle, philosophy of science is not a secular religion for conscience-stricken researchers. In this decade the question "Whither science?" has been posed *ad nauseum*. Divines, demagogues, and despondent dramatists have viewed science as the instrument of gleeful Frankensteins bent on creating the uncontrollable. And so they are led to "philosophize" about the future of our civilization. Doubtless, in an age of bigger and better bombs such questions are worth discussing—they are even worth discussing carefully, a thing which is rarely done. But no matter how well this is done, it is not an issue of internal importance to the teaching of science. It is a different subject. It affects scientists no more than it affects other members of the community. It is a matter affecting the scientist as citizen, not as scientist.

The author, an American who has been teaching in England for many years, is University Lecturer in the Philosophy of Science at Cambridge University. He delivered this lecture to the Education Group of the (British) Institute of Physics at the Institute's House, Belgrave Square, London, in April 1955. The lecture was first printed in the *Bulletin* of the (British) Institute of Physics, June 1955, and is reproduced here by permission.

* By Dr. Philip Frank of Harvard University, from whose writings our course at Cambridge has derived inspiration and guidance.

If there is a real case for the introduction of history and philosophy of science into undergraduate physics courses, this must consist in the possibility that men may become better physicists as a result. It is this stronger claim that I should like to consider. In any case, the speculative, deep-purple variety of discussion to which I have alluded finds no place in history and philosophy of science as it is dealt with at Cambridge.

Philosophy of science cannot, of course, increase manual dexterity. It is not unrelated, however, to the business of sharpening one's wits—the business, that is, of considering the character of one's experimental problems, the logical structure of arguments and proofs, and the general nature of a science's subject matter. The details of this I hope to set out in a moment. But I must first allude to another thing that philosophy of science is not, or need not be. For scientists often recoil at the sound "philosophy of science" for yet another reason.

Physicists rightly dislike the idea of academic philosophers and historians telling them and the world what science is all about. If physics were beset with all the problems that professional philosophers and historians manage to find in it, then doubtless they would be handy chaps to have around every laboratory, and every school and university concerned with the teaching of science. But here the scientist will ask, "How can scholars who are unlikely ever to have seen the insides of a modern physics laboratory—who have never muddled and groped through the perplexities of a research task of their own, or felt that profound unsettlement which attends every decision at the frontiers of physical inquiry—how can these individuals be relied upon to know what are the conceptual problems of physics?" Well, they cannot be relied upon, not unless they have been themselves scientists. Indeed, this is an indispensable requirement of anyone concerned with the teaching of these subjects. Unfortunately, it is not met by many individuals who expound on the history and philosophy of science.

The physicist's revealing question gains force when one sees how unrecognizable to researchers are some of the problems which "pure" philosophers have about the natural sciences. E.g.: How can one "construct" concepts of electrons out of visual impressions of pointer readings? How can one justify the use of inductive procedures in natural science? How can one, in the reports of his research, most closely approximate to the manner of exposition of the pure mathematician or the formal logician?

That these questions are not always recognizable to scientists does not entail that they are unworthy of being asked. Philosophers do ask them and they discuss suggestive answers to them often with great clarity and insight. But perhaps these problems are not recognized by scientists because they are not problems that scientists *have*, however much they might have had them had they but taken a degree in philosophy. Professional philosophers often refuse to discuss the problems physicists say they have, sometimes with the uncharitable aside that these are too naive for discussion. This is strange. For who should know better than the physicist himself what conceptual perplexities actually beset his laboratory work?

A connected consideration is the scientist's uneasy feeling that members of arts faculties see it as their mission to educate and humanize their underprivileged brethren, the physics researcher and the physics teacher. The vaunted gap between science and the rest of the academic community is to be closed, apparently, by plastering the Cavendish with culture, by threading modern poetry into the study of metallurgy and petrology, by hiding the intellectual nakedness of science graduates with a sheepskin fashioned as much by the dramatic arts as by laboratory science. Doubtless some of this would be a good thing, for there is such a gap. But it is a truth of geometry that gaps can be closed in three ways: by moving one extreme nearer to the other, or by moving the other closer to the one, or by moving both extremes simultaneously towards each other. It is a presumption on the part of arts people to complain of the low level of culture among the scientific fraternity when so few of the *literati* have even a rudimentary knowledge of the kind of thinking that has virtually shaped the modern western mind. The assumptions, methods, and attitudes characteristic of natural science since Galileo have had the most profound influence on our contemporary intellectual life. Without some understanding of these, an adequate assessment of the broad pattern of the life and society of our times is impossible. In this respect the physics student who has not read Homer is in a better position than the classics student who has not read Newton, or who knows nothing of mechanics. It is this feature of the work and thought of physicists which ought to be stressed from within the teaching of physics, at school and at university. Attention to the history and philosophy of science can be a powerful asset in this program. The rest of this paper will be devoted to making this more clear. For I have said much about what history and philosophy of science is not; now I must say what it is, and what it can be used to do.

ARE we all agreed that physicists are not just Tripos-tested laboratory machines? They have to think. But thinking always has a history and it always has a structure. To understand the history and the logic of an idea is to have a better grasp of that idea. It is to know what shaped the idea as it is. It is to see what are the implications of the idea, what observations are relevant and important to it, and what courses of action with respect to the idea are likely to be fruitful, as well as futile. Consider this further.

Try this question on your students: What is meant when it is said of some theorem of geometry—e.g. that the internal angles of a plane triangle equal a straight angle—what is meant by saying this theorem is *true?* Even casual reflection on the existence of alternative geometries will scare up the ancillary question: What is meant by saying of the actual space in which we live, the space in this room, that it is Euclidean? The question seemed natural for the founders of non-Euclidean geometries well over one hundred years ago. But your students are unlikely ever to have heard of the question. Indeed, the majority of university graduates in mathematics or physics have never heard of it. If you put the question to these graduates they will rarely understand its meaning without a thorough interpretation. This is startling when one considers that the question is of central and profound significance in any attempt to understand what geometry is the study *of*, and what mathematics is and what physics is.

Inspect any current textbook on geometry. See if you can find half a page devoted to the sense in which geometry is true. If you find anything at all it is likely to be an embarrassed attempt to dodge the question. This is criminal, for it is one of the most fruitful problems that could be put pertinent to the relation of physics and mathematics to other fields and methods of acquiring knowledge. Rarely is the student shown even so simple a thing as that mathematics is a formal science. It can make no pronouncement on matters of physical fact. It deals only with hypothetical statements of the form: *if* proposition A is true then it follows that proposition B is also true. Mathematics *per se* is never in a position to assert that the proposition A *is* true, in the sense in which we can say that the refractive index of diamond is 2.417 (for the λ of the Na D line). The statements of mathematics are of the form "all bachelors are unmarried". To deny such a statement is not false, it is absurd, meaningless. To deny a statement like "all bachelors are less than twenty feet in height", that denial would be false. But no such statements can occur in a formal science like mathematics or logic. For mathematics and logic are not concerned with what is, but with what follows.

This failure to explore even the surface of the foundations of geometry has an effect on the student which is devastating. Anyone who fails to grasp the relationship between mathematics and physics at this simple level will surely fail to grasp the relation between experimental confirmation and mathematical proof in more intricate domains of advanced physics. He is bound to misunderstand the role played by mathematical theory in quantum physics, in field theory, and in theoretical chemistry. A well-known university textbook asserts that Einstein proved "mathematically" that a material body cannot move with the speed of light. The most charitable thing one can say here is that the author has not expressed himself felicitously. But my suspicion is that he is unaware of the remark made by Einstein himself, namely that insofar as mathematics applies to reality it is not certain, and that insofar as mathematics is certain it does not apply to reality. How then could it be proved *by mathematics* that no body can exceed the speed of light? Most students of physics are not even given an instinctive feeling that no statements of physics can be proved mathematically, but that every so-called proof in natural science consists only in deriving (by mathematical means) one statement of physical fact from other statements of physical fact. When one surveys some of the recent history of quantum field theory it is impossible to avoid the conclusion that this instinct is not as strong amongst our bright young researchers today as it was for Einstein, Rutherford, Bohr, Dirac, Schrödinger, Heisenberg, and Born, men who have enriched philosophy of science by their acute analyses of which were the logical and which the experimental factors operating in their most important work.

The traditional "engineer's manual" approach to the teaching of physics has missed this opportunity to make clear to the student a distinction which is central both to the full understanding of his own science, and to many problems of interest in our society. In any field which could profit from a reasonable, scientific approach a central consideration must be the relationship between one's data—or evidence, or facts—and the conclusions which justifiably may be drawn from those data or facts. In a terse small-scale form we have here, in the distinction between pure geometry and the physics of actual space, a special case of a general issue concerning the interaction between experience and reasoning, between practice and theory.

WHY have we so long ignored this chance to add a new dimension to our physics teaching? Why have we been inclined to regard these considerations as something extrinsic to the actual business of teaching physics, to the business of getting the little blighters to trot out the correct formula at the correct time? It can only be that we have not ourselves been alert to the full meaning of the formulas. H_2SO_4 may not contain the whole history of mankind. But if it is carefully laid before the student he can come to feel what must have been the impact of this notation on chemical thinking in the 19th century, he can come to detect the important affinities, between operating and calculating with this set of symbols and operating and calculating with other symbols in other sciences. Indeed he can even come to form an idea of what a computational device can do in a natural science.

Why should he be denied this? If he is any good he will have to learn it for himself later. Even if he should drop out of science it cannot but have benefited him to have seen how experience and thought as they relate in physics is only a special case of the relation of experience and reason in every field of human endeavor. Certainly the failure to grasp this relationship accounts for some of the confusion felt by those perplexed with the complexities confronting them in private and in public life.

Thus the respective roles of mathematics and physics in our understanding of geometry is the simplest, most forceful example from the consideration of which the student can discern something of importance to his understanding of science in particular, and of problem-solving in general. In fact every problem in physics gives us such an example. Another is provided by Newton's laws of motion.

The first and second laws look very simple. Actually they are crucial issues for every teacher of physics. Their superficial simplicity makes any confusion in their presentation stand out in bold relief. A similar confusion in an advanced subject, like the three-body problem, passes unnoticed. But it is no exaggeration to say that 90 percent of the textbooks available for the first two years of university physics present the law of inertia so that its meaning is obscure. The words used in its formulation are applicable to no actual situation in the physical world. "An isolated body moving free of impressed forces is moving along a straight line." These words are jammed full of exciting questions, discussion of which is essential to any real understanding of the law. These are usually suppressed in favor of getting on with the symbolic Meccano. And the student receives a further encouragement to regard mechanics as a dead catalogue of formulas rather than, what it was for Galileo, Newton, Hooke, and Laplace, the most thrilling and spectacular of intellectual achievements.

What *is* an isolated body? Do we have any concept at all, much less a clear concept, of a body moving free of impressed forces? Aristotle, whose genius no one would question, took a different view of the matter. So its truth cannot be self-evident or obvious. For Aristotle, undisturbed bodies in motion always came to rest. This is not bad physics. Observation and experiment are largely in support of Aristotle. If one of your bright students were to adopt the Aristotelean position here, how would you bring him 'round? What would you point to, what could you show him? Indeed, just what considerations compel our assent to the first law? Once posed, the question opens the whole fascinating business of theory-construction before us.

What a revelation it can be to a student to learn that we accept the law of inertia for just the sorts of reasons that we accept atoms, and electrons which spin and move in a manner formally analagous to the movement of a wave group, and neutrons, and positrons and neutrinos. We cannot just look and see any of these things directly. But the student will learn that this does not matter. By assuming these things, the law of inertia, electrons, neutrinos, many of the things we do observe directly can be clearly and powerfully explained. What better reason could there be for assuming them? Not one schoolboy or undergraduate need persist in regarding mechanics as a text of dry lessons which must be mastered before he can get on to "the exciting stuff". For classical mechanics is physics. It involves decisions of great importance for every concept built upon it. It is riddled with ideas and unanswered questions of exactly the same type that punctuates contemporary physics. The earlier the student comes to feel the fundamental, philosophical character of the concepts with which he must deal, the better able will he be to make his own decisions and form his own views when he stands at the frontiers of a problem where there are no hackneyed textbooks to guide him or misguide him.

Let the student know that Mach regarded the first law as an unnecessary tautology completely contained in the meaning of the second—and let the student discuss this. Let him know that for Mach the empirical content of the second law lies in our ordinary experience of forces and tensions in our muscles. For Hertz, on the other hand, the second law is only an implicit definition of the terms concerned; let the student discuss this, after reminding him that Maxwell himself first speaks of the laws as pure definitions and then proceeds to list the experimental evidence in their favor. This is rather like asking the mother of a newly born child what evidence she has for calling him "James".

Soon the student will come to see that though these matters are the conceptual glue which holds all of physics together, there are here no neat, tidy answers, not in the sense that there are answers to the specific questions which this glue makes it possible to ask. Nothing can match such discussions for destroying the schoolboy conviction that physics is a great shelf of thicker, more unreadable textbooks and directories, all containing the right answers in the back pages. If science were just such a shelf then there would indeed be an unbridgeable gap between it and other disciplines. No student with initiative or imagination would dream of undertaking such a study.

CONSIDER the first law further. It is usually presented in the language I used a moment ago. But clearly, "moving along a straight line" is an expression without physical meaning unless some reference system is physically specified, a system in which a straight line is defined and fixed so that it can serve as the standard by which we judge whether a given motion is rectilinear. In current textbooks one rarely finds even the perfunctory statement that the system of reference needed in the first law is the system of the (so-called) fixed stars. Thus the law as stated in the textbooks lacks even a vague meaning since no method is even suggested whereby it can be considered in a concrete case.

So long as the nature of the verification of a physical law remains obscure, the way is clear for every opinion as to its character and validity. In some books the law of inertia is taken to be self-evident and Aristotle's dictum to the contrary is ridiculed. Others regard the law as not at all self-evident; they say that it is confirmed in familiar experiences, such as that a chair will rest immobile on the floor unless we lift it or push it. Still other books construe the law as a theoretical hypothesis, no more, no less. It cannot be proved by any experiment, but it is not for that reason self-evident. *The law of inertia is really all these things.* It can be used in a variety of ways in different contexts. If a physicist thinks he is checking the first two laws by the use of Atwood's machine then he is clearly using the laws differently than when he employs them as inference schema in celestial mechanics, or when he uses them in calculations concerned with the construction of apparatus. Most books try to settle the issue once for all. This is rather like trying to settle once for all what is the use of a rope, or a plank of wood. Laws of motion, like planks and ropes, have as many uses as there are contexts in which they can be used. Of all people the writers of physics textbooks should know this.

Of course the issue here is nothing less than the character of man's knowledge of nature, an issue which the science student ought to be better able to understand and discuss than the arts student. But it is just this understanding and discussion which is suppressed in the standard, stern, omniscient presentation of the subject. The result is that science teaching loses another opportunity to reveal the full importance of physical thinking. The "gap" of which educators never tire of talking remains, whilst well-meaning arts teachers try to sandwich thin slices of Shakespeare between thick crusts of statics and Snell. Physics teaching does not need this superficial humanizing from the outside. It ought to be an inside job. For physics is itself the most human of accomplishments: man's relentless struggle to form a systematic set of conceptions about the world in which we live. Were this potentiality of physics teaching realized it would soon be obvious that the serious gap lies in the ignorance of arts students to the elements and principles of that science which has to a remarkable degree determined what today's men of letters think about.

Search in a physics text for an evaluation of the law of inertia. What do you find? "It is amazing that it took so many centuries to discover such a simple and trivial law." If it was so simple and trivial why did it take a Galileo and a Newton, nothing less, to offset the views of Aristotle (who was at least as capable as these modern textbook writers of discerning the simple and the trivial)? By simplifying and trivializing these laws we lose yet another opportunity to disclose the achievements of early physicists. And we are in danger of promulgating the parochial idea that only the research of our own day is difficult, exciting and intellectually challenging.

Think yourself into Galileo's context. Try to come to appreciate the effort it must have required even to form the theoretical concept of acceleration. That effort was at least as great as that required of Planck, Einstein, Compton, and de Broglie in their individual achievements of 1900, 1905, 1923, and 1924 respectively. How wrong we are to let it sound as if these latter were discoveries requiring some different kind of intellectual effort than did the laws of acceleration and inertia. When a teacher calls fundamental laws of classical mechanics "simple and self-evident" he has not himself understood their real significance, either for physics or for human history.

If elementary texts formulate the first law in elaborate but meaningless ways, advanced texts seem proud to minimize it altogether. In one such book the law is called "self-explanatory". This saves the author from having to explain it, since the law is busy explaining itself. He then treats the law as a special case of the second law, as do most writers: if ΣF is 0 then a is 0 too, and the body moves with a constant velocity. The difference between this and what is said in the elementary books is that here it is not the first but the second law which is formulated in an obscure way.

Ask any graduate student what "acceleration" in this law means. He will say that the acceleration is the second derivative of the coordinates with respect to time. Then ask what he means by "coordinates". He will make the usual reference to a Cartesian coordinate system. But a motion of a body is described *physically* only if it can be specified with respect to a system consisting of observable physical bodies. An operationally sound account of what is meant by "acceleration" in the second law must describe a physical Cartesian system. This cannot be in fixed connection with our earth. The fixed stars may serve as a first approximation. If that is so then acceleration is (approximately) "with respect to the fixed stars of our galaxy". Even PhD candidates often show little awareness that the unqualified expression "fixed in space" may be meaningless in physics. Persistent questioning will soon reveal to them that they have not thought nearly enough about the simplest laws of their science.

Failure to describe the physical system of reference makes little difference to the engineering student. No bridges of his design will collapse because he never heard of the difficulties involved in the concept of acceleration. Were this innocence preserved in physics however, it would soon be obnoxious. The fixed stars do not form a rigid Cartesian system. So they cannot replace the abstract, purely formal term "Cartesian system". Here is a situation analagous to that mentioned earlier concerning the applications of pure geometries to actual spaces. Whether there is a physical space which can be described by Euclid's geometry is a matter for experiment, observation, and inquiry to decide. It is logically possible that we might be wrong in saying of any actual space, e.g., the space in this room, that it is Euclidean in every respect. Similarly, whether there is in the universe a physical Cartesian system with reference to which the laws of Newton's *Principia* can be observed to hold is also a matter for experiment, observation, and inquiry to decide. Again, it is a distinct possibility that we might be wrong in saying of any actual portion of the universe—e.g., the matter enclosed by the orbit of Pluto—that its behavior is Newtonian in every respect. So we must say that the positions and velocities of the fixed stars, and even of the remote nebulae, determine in some way the system of reference intended when the term "acceleration" is used without further specification. Thus our rolling billiard ball is physically influenced by the states of the stars in our Milky Way. These, and the galaxies millions of light years distant, determine the motion that we call "motion under no force".

Here is a startling and exciting fact about the world. One can only learn it through physics. This makes the suppression of the fact by textbooks and teachers difficult to understand. By ignoring the influence of the large but remote masses of the universe, the mysterious concept of absolute space was smuggled into physics, to the consternation of everyone who had later to fight off its influence. Yet we are at no great pains today to insure that the concept never gets a grip on the student. Perhaps later the effort required of him to fight off this idea will be good for his soul. That may be true. But as a pedagogical technique it is of at least debatable value.

Foundations of geometry and the laws of motion: these are just two of a large number of crucial concepts in physics the free discussion of which can deepen the student's understanding of the kind of knowledge science can give him, and of the fundamental ideas which hold physics together. This is not something to be tacked on to physics. It is not a sugar-coated afterthought, something to disguise the hard work that must be mastered one way or another. It will develop from the actual teaching of physics if allowed to do so.

BEFORE concluding I should like to mention how Dr. Hall and I proceed with the undergraduates at Cambridge.

It has been essential that we have a pretty good idea of what the undergraduates are doing from week to week in their science lectures and laboratories. When I first went to Cambridge from "the other place" I devoted most of my mornings to attending the physics lectures with my students. My own lectures were designed accordingly. When Professor Sir Lawrence Bragg was lecturing on optics, he dwelt in detail with interference phenomena, and he raised interesting general questions about the controversy as to the nature of light. At just this time Dr. Hall was considering what inclined Newton towards holding a corpuscular theory. He was dealing also with Huygen's recommendations in favor of the undulatory theory. Hall is always careful to stress the essential opposition in *ideas*—the intellectual content of such disputes. Students' reactions showed that this historical excursus complemented Sir Lawrence's lectures admirably.

Then Hall moved to a treatment of the conceptual context in which Young, Fresnel, and ultimately Foucault conducted their famous experiments. At this point I had been lecturing for about a week on the general question of crucial experiments in physics—what they are, whether they are desirable, and when they are possible. So it was natural to consider these experiments further, for they are all described as "crucial". If they were crucial we should never again have heard of the corpuscular theory of light. This has spectacularly proved not to be the case. Exploring further, we found that these 19th century experiments were indeed crucial, but only against an implicit background of assumptions like this: Young, Fresnel, and Foucault all accepted the prior proposition, "Light must propagate itself in either (1) a wave-like manner, or (2) in a particulate manner, but (3) in at least one of these ways, and (4) in no case both of these ways at the same time." There was no justification for holding to (3) and (4), though as Hall shows it was then natural to do so. Yet it is equally clear that these experiments could be regarded as crucial to deciding the nature of light only if the logical condition expressed in the clause ". . . but in at least one of these ways and in no case both of these ways at the same time" were accepted without question. The discoveries of this century have shown not that light is *not* really undulatory after all, but that Young, Fresnel, and Foucault were perhaps too hasty in accepting of what is expressed in this important but seldom noticed clause.

So it is with all so-called crucial experiments. They are crucial only against a background of theory and presupposition which must be beyond dispute so long as the experiment is still regarded as crucial. Question these presuppositions and the forbidden apple is bitten. It becomes immediately clear that the finality of the word "crucial" is bought by mortgaging away all expectations that anything new may yet be discovered which will change our ideas as to the character of the phenomena before us.

These historical and philosophical discussions provided a rich context for Sir Lawrence's lectures. It was clear that everyone concerned profited from this liaison. Hall and I resolved to let the science lectures provide the texts on which we would preach.

Similarly, Mr. J. A. Ratcliffe lectured in part I mechanics last year using Ernst Mach's book *The Science of Mechanics*. As you know, Mach is a gold mine of historical and philosophical nuggets. We lost no time in meshing our work with Ratcliffe's lectures. Hall was then discussing Galileo and Newton and the great advances they brought to mechanical thinking. I was examining the logical structure of a physical theory like the mechanics of Newton or of Hertz; I explored also the logical nature of the principles, laws, generalizations, and hypotheses which must figure in any such theoretical system.

Threading our questions into the actual day-to-day teaching of the straight physics lectures has been effective. Enthusiasm has sprung up at Cambridge, amongst dons and undergraduates alike. Our philosophy of science club this year recorded 250 members of which number some 50 were of senior standing in the university. Plans to advance and intensify work in the history and philosophy of science within the natural sciences are now at an advanced stage at Cambridge, with the support of almost all of the major scientific faculties.

It is too early to say what will be the long term effect of this deepening of science teaching—notice that I say "deepening" rather than "broadening". The broadening will come naturally when the fundamental aspects of physics are exposed and discussed in schools and universities. For of all man's efforts to make his surroundings intelligible nothing can approach physical inquiry as an exciting, formally beautiful, and intellectually challenging exploration. Anything that can bring these aspects of physics into better focus is worthwhile. More attention paid to the historical and philosophical aspects of physics might serve as a lens for the achievement of this better focus.